THE AËRONAUTICAL JOURNAL.

APRIL, 1903.

The Aëronautical Journal.

(PUBLISHED QUARTERLY.)

SUBSCRIPTION PER ANNUM (post free), 4s.

Communications respecting Advertisements to be addressed to the Publishers, "Aëronautical Journal,"

Messrs. KING, SELL & OLDING, Ltd., Lonsdale Chambers,
27, Chancery Lane, W.C.

Editorial communications should be addressed to the Editor,

53, Victoria Street,
Westminster, London, S.W.

NOTICES OF The Aëronautical Society.

At a meeting of the Council of the Aëronautical Society of Great Britain held at the offices of the Society, 53, Victoria Street, Westminster, on Monday, February 2nd, 1903, the following gentlemen were elected members of the Society:—

MR. ERNEST EDWARD BARCLAY.
MR. GRIFFITH BREWER.
MR. CECIL CHARLES BRINTON.
MR. ARTHUR CHURCHMAN.
MR. CHARLES BROGDEN.
MR. JOHN GALLATHY.
MR. FREDERICK KNOTT.
MR. CHARLES F. FLORA.
DR. BLAZIUS REKA.

At a meeting of the Council held at the Society of Arts, John Street, Adelphi, on Tuesday, March 24th, 1903, the following gentlemen were elected members of the Society:—

SIR DAVID SALOMANS, BART.
CAPTAIN ROBERT BROCKLEHURST.
ALAN HUGHES BURGOYNE.
BERNARD D. LANG.

INTERNATIONAL KITE COMPETITION.

The concluding meeting of the thirty-eighth session of the Aëronautical Society of Great Britain will be held probably on the Sussex Downs on June 25 to witness the International Kite Competition for the Silver Medal of the Society, which will take place on that day (wind and weather permitting).

Due notice will be given to members of the exact site selected and other arrangements.

The following are the general rules drawn up by the Council of the Aëronautical Society of Great Britain:—

1. The Competition is for the highest flight attained by a single kite beyond 3,000 feet.
2. Duration of flight to be one hour, and kites must be sustained in the air during this period.
3. Each kite must carry a weight of two pounds. The weight to be placed within 100 feet of the kite.
4. The height to be determined trigonometrically from the ground.
5. The Competition is open to all approved by the Council of the Aeronautical Society of Great Britain, without limit of form of apparatus, and each Competitor must provide his own apparatus, winding gear, etc.
6. Each Competitor is to be held responsible for any damage or accident

that might occur to or by means of his apparatus.

7. Members of the Aëronautical Society of Great Britain will have to pay no entrance fee, but for non-members there will be an entrance fee of half-a-guinea.
8. Not less than two Competitors can enter for the Competition.
9. The Jury will have the decision whether the Medal is to be awarded.
10. The decision of the Jury must be accepted as final.

In issuing these General Rules the Council reserve to themselves the right of making any additional regulation which may tend to facilitate the carrying out of the Competition.

Amongst those who have kindly consented to act on the Jury are—Mr. C. V. Boys, F.R.S., Mr. E. P. Frost, J.P., D.L., Sir Hiram Maxim, Dr. Hugh Robert Mill, LL.D., F.R.G.S., Dr. William Napier Shaw, F.R.S., Mr. Eric Stuart Bruce, M.A.

Members of the Aëronautical Society of Great Britain and others who wish to take part in the above Competition should communicate with the Honorary Secretary of the Aëronautical Society of Great Britain, 53, Victoria Street, Westminster, London, S.W.

Competitors are requested to make their weights of lead and fix them so that they may be easily detached for weighing.

On hearing of the death of Mr. James Glaisher, honorary member of the Council of the Aëronautical Society of Great Britain, the Society despatched a floral wreath, with a telegram of condolence to the family of the late Mr. Glaisher expressing their sorrow at the loss of their distinguished member.

ANNUAL SUBSCRIPTIONS.

Members of the Aëronautical Society who have not yet paid their subscriptions for the current year are reminded that their subscriptions became due on January 1st last, and are requested to forward the same to the Honorary Secretary, 53, Victoria Street, Westminster. The attention of members is called to Rule IVA, by which the privileges of members only extend to those who have paid their subscriptions for the current year.

ERIC STUART BRUCE,
Honorary Secretary.

THE LATE MR. JAMES GLAISHER.

On February 7th, Mr. James Glaisher breathed his last. Many, indeed, of the giants of science who, in their several departments, illumined the Victorian era ceased to be with us before the brilliant century ended. But to Glaisher, who attained the age of 94, it was granted to witness the dawn of the new century. This, perhaps, is distinguished more for mediocrity and unrest than for the earnest, sustained and prolonged efforts by which the scientific workers of the 19th century raised up their lasting monuments.

Great, indeed, is the debt of gratitude that the Aëronautical Society of Great Britain owes to the memory of the illustrious scientific aëronaut, who was a member of its council from the date of the formation of the Society, in 1866, to the day of his death, and an active worker in its interests until extreme old age necessitated his retirement. It was, indeed, Glaisher who made the first speech before the Society at the preliminary meeting of its council held at the residence of the late Duke of Argyll on the 12th of January, 1866. In this address he eloquently urged the important claims of aëronautics as a science, and deplored the inadequate attention that scientific men had bestowed upon the science. He expressed in that speech the hope that "by the organisation of the Aëronautical Society of Great Britain the subject may take its standing among the sciences, for it has been found that societies formed for the study and advancement of special branches of science, if properly conducted, have invariably been productive of most important results, and have been the means of developing and bringing forward much that is valuable in the way of invention and discovery; and the council may expect that by avoiding all public display, and rigidly excluding any matter that may seem useless in forwarding the science, or that has the slightest chance of giving rise to ridicule, the exertions of individuals directing their ingenuity in this channel may be duly appreciated, and that all information concerning the science of aëronautics may be brought to a focus, and made available through the medium of the Society."

It may possibly have been some satisfaction to the scientist who gave utterance to this opinion in 1866 to know in later years that he was fully justified in his hopes of

the future of the British Society that is devoted to the study of aeronautics. That so far from the subject being disparaged or despised, it now has the warm support of men of light and teaching in every branch of science.

But there is another scientific society, one whose work is often bound up with that of the Aëronautical Society, that is indebted to Glaisher for its solid foundation; that is the Royal Meteorological Society. Besides helping to found this Society in 1850, when it was called the British Meteorological Society, he also became its organising secretary. How much of the later prosperity of this Society, and how much of the extension of meteorological science throughout the world is due to the thorough exercise of his office would be difficult to estimate.

James Glaisher was born in London on April 7th, 1809. When 20 years of age he was appointed an assistant on the principal triangulation of the ordnance survey of Ireland, in which capacity he had charge of the meteorological observations on the Bencor Mountains in Galway, and, later, on the summit of the Keeper Mountain, near Limerick.

It was this appointment which developed his taste for aerial physics. "In the performance of my duty I was often compelled to remain sometimes for long periods above or enveloped in cloud. I was thus led to study the colours of the sky, the delicate tints of the clouds, the motion of opaque masses, the forms of the crystals of snow. On leaving the survey and entering the observatory of Cambridge and afterwards that of Greenwich, my taste did not change. Often between astronomical observations I have watched with great interest the forms of the clouds, and often, when a barrier of cloud has suddenly concealed the stars from view, I have wished to know the cause of their rapid formation and the processes in action around them." No doubt it was these early surroundings that fostered in his mind the idea that better still than the mountain-top for cloud observation would be the balloon, which would float with, above, or below the ever-changing surfaces of clouds, and determined those future and classic scientific balloon ascents which have added so materially to our knowledge of the atmosphere.

From 1833-1836 he was an assistant at Cambridge University. In the latter year he was appointed an assistant to the Royal Observatory, Greenwich. There his worth was soon recognised, and in 1840 he became superintendent of the magnetical and meteorological department.

But the portion of his work which specially claims the notice of this journal is his achievements in aerial exploration. For the scientific results obtained these undoubtedly rank the highest in the history of scientific aeronautics, though his past labours are being now so efficiently supplemented by the work of Herren Berson, Assman, Hergesell, and others, in Germany. Mr. Glaisher made no less than 28 ascents in the interests of science, the results of which will be found in the Reports of the British Association for the advancement of science, 1862-66. Of these, seven were specially high ascents. Six others were undertaken for the objects of the committee alone. Four of these ascents were undertaken from Wolverhampton, and seven from Woolwich. It was in the year 1859 that the committee appointed by the British Association to make observations in the higher strata of the atmosphere met at Wolverhampton, but only with the result of a few desultory efforts, for the volunteer was then wanting who would concentrate his energies on the task and risk his life in its accomplishment. It was in 1862 that Mr. Glaisher, who was a member of the committee, offered to be the investigator, and set to work on the preparation of the apparatus with which the car of the balloon had to be equipped. A complete and popular account of Mr. Glaisher's work appears in that volume edited by him and entitled "Travels in the Air." It must suffice now to mention a few remarkable features of those famous ascents which he made in company with Mr. Coxwell, to whose masterly piloting the success of the ascents was so largely to be attributed.

Until the ascents of Mr. Glaisher it had been thought that the decrease of temperature in ascending was constant, that it was at the rate of 1° for 3,000 feet of elevation. But observations taken by Mr. Glaisher first proved that no such uniformity exists, that the temperature of any point in the more immediate atmosphere depends upon many circumstances; for example, the season, the time of day, a clear or a cloudy sky, the direction of the aerial currents. Many surprising changes of temperature were encountered by Mr. Glaisher during his high ascents. In fact, his very first scientific ascent, from Wolverhampton, on July 17th, 1862, was remarkable for meeting with a

warm current at a considerable elevation. On the occasion of this ascent the temperature of the air at starting was 59° Fahr., at 4,000 feet it was 45°. It descended to 26° at 10,000 feet, from which height to that of 13,000 feet there was no diminution of temperature. While passing through this space Mr. Glaisher put on additional clothing, feeling certain that a temperature below zero would be attained before the height of five miles was reached. But at the elevation of 15,500 feet the temperature was 31°; at each successive reading up to 19,500 feet it increased, and was then 42°. The temperature then decreased rapidly, and was 16° at 26,000 feet.

The limit of human endurance to the conditions of rarefaction was unknown until Messrs. Glaisher and Coxwell carried their heroic exploration to the very verge of the boundary-line in their famous seven-mile ascent of September 5th, 1862. Very striking were the different effects of the rarefied atmosphere on these two explorers. At the height of seven miles Mr. Glaisher was in a swoon, paralysed from head to foot, while Mr. Coxwell was fully conscious though he could not use his hands, and had to grasp the valve cord with his teeth to open the valve for descending. Doubtless the possible limit was nearly reached on this memorable occasion, though individual constitution, acclimatisation by repeated ascents, and the respiration of oxygen might possibly slightly extend the boundary line.

The ascent of April 18th, 1863, from the Crystal Palace was remarkable for the rapidity of the descent, and was an example of the presence of mind invariably displayed by Coxwell. At 2 hrs. 44 mins., the balloon being then at a height of 10,000 feet, the aeronaut suddenly caught sight of Beachy Head, and Mr. Glaisher, looking over the edge of the car, saw the sea immediately underneath. There was no time to be lost, both Glaisher and Coxwell hung on to the valve line. The earth was reached at 2 hrs. 48 mins., the two miles of descent having been effected in four minutes. The balloon struck the earth at Newhaven with a terrible crash, and all the instruments were broken.

In the course of the ascent on June 26th, 1863, from Wolverhampton, rain clouds were found to extend, layer above layer, to a height of four miles—a fact hitherto regarded as impossible. In Glaisher's high ascents the existence of the varying air currents in different directions at different elevations was very manifest. The thickness of current was found to vary immensely; the direction of the wind on the earth was sometimes that of the whole mass of air up to 20,000 feet, whilst at other times the direction changed within 500 feet of the earth.

Of the strength and direction of these varying currents no quantitative observations have yet been recorded, and by what means they can be quantitatively recorded, since the balloon moves with the aerial current, is a problem for future investigation. But it is a most important one, not only in the interests of meteorology and weather forecasting, but also of aerial navigation.

The power and value of individuality was strikingly exemplified in the work of the great aeronaut whose loss we mourn. After Glaisher ceased to explore the atmosphere, for years there was even no attempt to continue the task he so well commenced. Though the task is an all important one for the progress of meteorology and science generally, we have been leaving other nations to follow the leader whose memory is our glory and pride. It is, however, now the resolve of the Aëronautical Society of Great Britain to take up the work of its first chief promoter and guide when funds can be procured adequate to carry out the necessarily expensive work of aërial exploration.

Mr. Glaisher was elected a Fellow of the Royal Society in 1849, and was a member of several learned bodies. He had the honour of acting as President of the Royal Meteorological Society, the Royal Microscopical Society, and Royal Photographical Society. He was also for twelve years chairman of the Palestine Exploration Fund.

Amongst his contributions to science are several papers in the Philosophical Transactions, Hygrometrical Tables (1847), "Travels in the Air," which, as has already been mentioned, was edited by him, and containing the account not only of his own valuable balloon voyages, but also of his followers in France—M. Tissandier, Flammarion, and De Fonvielle; a book unfortunately out of print and hard to procure —Diurnal Range Tables: Mean Temperature of every day for Greenwich, 1814-1873: Reports on the Meteorology of Palestine and India. In the annual reports of the Aëronautical Society of Great Britain, which preceded the present quarterly "Aëronautical Journal," will be found many interesting speeches and remarks by Mr. Glaisher on those many occasions when he presided at the general meetings of the Society.

GENERAL MEETING.

The second meeting of the Thirty-eighth session of the Aëronautical Society of Great Britain was held at the Society of Arts, John Street, Adelphi, on Tuesday, March 24th, 1903. The President, Major B. F. S. Baden-Powell, occupied the chair.

The CHAIRMAN: Ladies and Gentlemen,—The proceedings will commence by the Honorary Secretary reading the Minutes of the last two meetings.

The HONORARY SECRETARY read the Minutes.

The CHAIRMAN: Ladies and Gentlemen,—There is one matter which I should like to bring forward to-night. We must all very much regret that since our last meeting we have lost one of our most prominent members; I refer to Mr. Glaisher. He is so well known that I need hardly refer to what he has accomplished in the aëronautical line, but though his name is very well known, he was specially known in this Society, having presided on a great number of occasions at our meetings. The Honorary Secretary tells me that his name appears 41 times in the book as having signed the Minutes at different meetings, and that shows what a keen interest he took in the proceedings of this Society, and I feel that the least we can do is to propose a vote of condolence to the family of Mr. Glaisher. I need not detain you any longer as we have a good many papers to get through to-night, but I will ask Major Roberts if he will second this motion.

MAJOR ROBERTS: I have great pleasure, ladies and gentlemen, in seconding this vote of condolence to the family of the late Mr. Glaisher. I may take this opportunity of reminding you of the memorable ascent which he made with Mr. Coxwell in 1862, when they reached the unprecedented height of 37,000 feet, just over seven miles. Between 1863 and 1869, Mr. Glaisher made, altogether, 29 balloon ascents, all in the interests of science. He was, as we know, a Fellow of the Royal Society, of the Royal Microscopical Society, and of the Photographic Society, and he founded the Meteorological Society. We may regard him, in his particular line, as one of the great men of the past century.

The vote of condolence was carried unanimously.

The CHAIRMAN: The paper now to be read is one by Mr. John Anderson, on "The Kite Equipment of the Scottish National Antarctic Expedition." Mr. Anderson is now abroad, and his paper will be read by the Hon. Secretary.

"The Kite Equipment of the Scottish National Antarctic Expedition."

By MR. JOHN ANDERSON.

We reproduce photos of one of the kites to be used for meteorological purposes, and of the reel and motor by which the kites will be controlled when in the air, which forms part of the equipment of the Scottish National Antarctic Expedition, which lately sailed from the Clyde, in the *Scotia*, for the South Polar regions, under the leadership of Mr. W. S. Bruce.

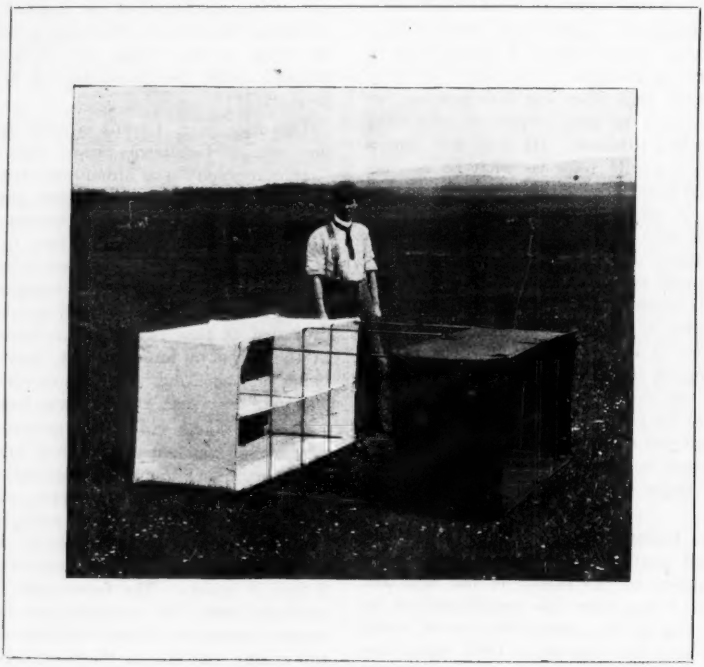
This kite (Fig. I.) has already lifted into the air at Leadburn, near Edinburgh, a "meteorograph" (an aluminium box containing a barometer, thermometer, and hygrometer, with recording drum driven by clockwork—the whole weighing 3 lbs.) to a height of over 6,000 feet, and carried it steadily at that height for many hours, bringing back a record of the temperature and moisture which prevailed at that height. The box was hung in the air at a hundred feet from the kite, being attached by a clamp to the wire or "string," by which the kite was flown.

The kite has a sail area of something under 80 feet, of which nearly 70 feet always faces the wind, being the lifting surface, while the remainder faces the sides, serving to hold the kite in a steady position, and acting as a helm. It stands 6 feet 8 inches high, and being equally broad, is a square; while its depth is 2 feet 8 inches. The framework is of light bamboo canes. It is really two flat kites—6 feet 8 inches by 6 feet 8 inches—one behind the other; the kite at the back being held at a distance of 2 feet 8 inches from the one in front by small cross canes; but the sails of the front kite are carried round and joined to those of the back—this extension of the sails along the sides having the effect as above stated of giving steadiness to the conjoined kites. It will be observed that the whole surface within the kite framework of bamboo, 6 feet 8 inches square, is not covered with sail, as in the old familiar form it was, but a space in the centre, equal to a third of the total area, is left uncovered. By leaving this space blank, the sail area is divided into two equal parts—an upper and a lower—the lower half acting as a counterpoise to the upper, being, in fact,

a tail, but still furnishing its quota of lifting power, although in a somewhat diminished degree as compared with the upper.

With a view to increase the lifting power, it has been found advantageous to introduce a middle sail in the upper half (as shown in the photo), leaving the lower half as before, to act as a counterpoise. The introduction of the middle sail is really the starting of a third kite in series, but a kite all lifting power and no tail. This sail is at a distance of 1 foot 4 inches from the sail on either side. This space is ample for the wind to act fully on the sails arranged thus in series behind each other, where the depth of the sails is about 2 feet, because the kite, when flying, assumes a very flat position in the air, making an angle with the horizon of seldom more than 20° to 22°, which thus prevents the sheltering from the wind of one sail by the other.

The upper sails are white and the lower black, with a view to one or other being visible, whatever the colour of the background made by the clouds—the white showing out against black clouds, or vice versa. The interior of the kite is closely intersected by steel wires which brace it in every possible direction. This has the disadvantage that the kite is not readily collapsible, and so requires space for stowing away; but it makes a kite which will retain its shape, however strong or erratic the winds, and can safely be counted upon to fly steadily up to the limit of the strength of the cloth, bamboo, and wire.



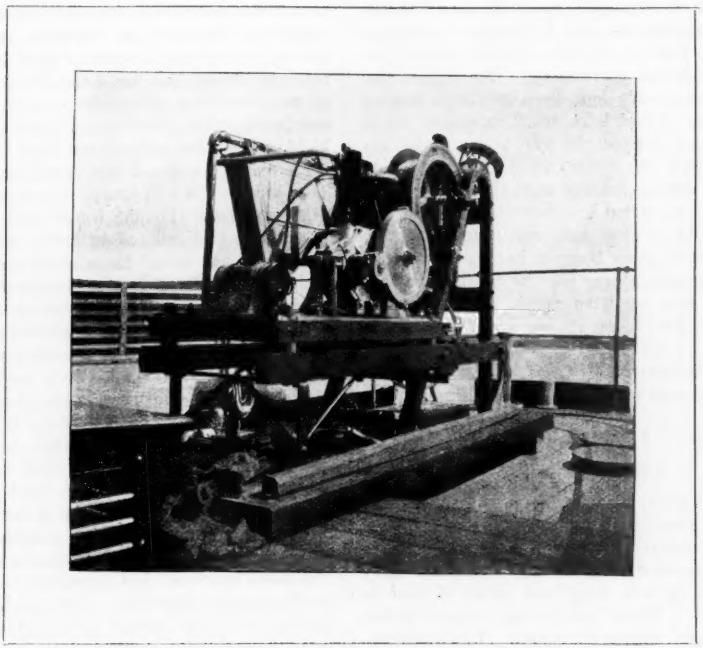
The Kite Equipment. (Fig. 1.)

The weight of the kite is about 10 lbs., and the length of the wire used for bracing purposes is nearly 400 feet.

Steel piano wire forms the flying "string" of the kite, and the photo now shown on the screen (Fig. 2) shows the mechanism employed to handle it. The motor is a 2½-h.p. ordinary petrol motor with electric ignition, such as used in cycles, but with the parts re-arranged on a stout mahogany bed, and joined up to the winding apparatus by a sprocket chain. The motor is not used when the kite is ascending.

but can reel in at any desired speed up to four or five miles an hour against the pull of the kite, which is indicated on a large dial, by watching which the engineer judges what speed to use. The number of feet of wire out is shown on another dial,while a pointer follows and indicates the angle of the kite's height. The wire enters the apparatus through a tube which revolves on ball bearings, giving the kite a play of more than half a circle, and before passing on to the storage drum passes over a wheel which distributes it equally over the surface of the drum. There are a set of tension wheels which take the strain of the wire, thus relieving the storage drum of all but a pressure of a few pounds, which, but for these wheels, would be liable to be crushed owing to the cumulative nature of the pressure produced when successive layers of wire under continuous pressure are stored on a drum. There are suitable levers for regulating speed, friction clutch for connecting or disconnecting motor, etc. The diameter of wire used is .032 inches, and its breaking strain 300 lbs. The drum can store seven miles.

Experiments conducted in July on the Clyde from the S.Y. *Mermaid* with this apparatus showed that, when kite-flying is done from the deck of a steamer, the vessel's engineer can be made to assist very materially. The speed of the wind can be increased or lessened by altering the course or the speed of the steamer, and when the wind was fairly strong it was found the kite could be kept flying with the vessel going before the wind as well as in any other direction.



The Kite Equipment. (Fig. 2.)

After the reading of this paper the Honorary Secretary exhibited on the screen two photographs; (1) taken from the kite wire showing a road with a wall on each side, and a ploughed field with patches of barren ground; (2) showing the engine and the kite in the air. Both these photographs were taken on the Braid Hills, near Edinburgh.

The CHAIRMAN: Mr. Anderson's son is present, perhaps he will say a few words.

Mr. Anderson: I have not very much to add to the paper. I was only present at one of the experiments. It is evident from the photographs shown on the screen, which were taken from the kite, that the camera has moved slightly during the exposure. The camera had a focal plane shutter, but the method of letting off the shutter was at fault. The trigger was attached to the camera and hung well below, and was on the same level as the wire, so that when a message was sent up a tin disc with a tube fitting over the wire was allowed to slide up the wire by the force of the wind, and it struck the trigger and set off the shutter. But the speed it obtained was rather considerable, and I imagine the camera felt it. That, I believe, is the reason the photograph was not clear. If a thread had been used, and a long fuse such as a mining fuse, there would have been no shock, and it would have allowed the kite to get up to any height before the camera went off, but it would be more difficult to time with the arrangement we had. We tried the American kites of this type. We copied the specification of the American Weather Bureau, but we found they were too light, at any rate for Scotland, at the time of year we tried them. They went to pieces. The kites we used were somewhat heavier.

The Chairman: Mr. Alexander will now describe his new invention, "The Aërosac."

"THE AËROSAC."

By Mr. Patrick Y. Alexander.

It has been the custom in Japan for the last 500 years to hang these objects, shaped like fish (referring to diagram on screen) over certain houses during the month of May. They are from 30 feet long and about 6 feet in diameter. There are several curious points about them worthy of notice. First, one observes the fine threads that are holding the fish. This gives rise to the thought that the head resistance is small. In fact this is the case, there are three threads and they are very fine, indeed. The fresh air blows in at the mouth. Several of these have been made up in very simple form, being merely bags with the end drawn in something like a pillow. The name "Aërosac" has been suggested by Mr. Groombridge, and I consider it a good name. As kites, they do not fly high, but possess remarkable stability. They are cheap to make, and have very little pull, in fact, at times, they have been observed to float almost by themselves. They are also extremely portable, and are almost instantly extended by the wind. They are easily packed, and may be stowed away in a small compass.

The Chairman: Mr. Alexander is very modest, and he won't tell us as much as he could about these things. Now, I happen to know something about them, because I have been down to see Mr. Alexander experiment with the Aërosac, and I have seen what he can do. I know he has gone into the matter very scientifically, and he has had some big—I don't know whether we can call them machines—but big pieces of apparatus somewhat similar to the fish which you have seen on the screen, and he has made careful tests with these in order to find out the amount of resistance that the air blowing against the opening at the end presents. And there seems to be no doubt that for some reason which I do not think Mr. Alexander can explain very accurately, the wind has a peculiar action. It blows into the hollow, and then it seems to come out again round the edges and in some way lessens the resistance, which one would otherwise have thought would have occurred in a large box inflated with the wind. Mr. Alexander has one of these machines, which I suppose is about 6 feet in diameter, and we made rather a peculiar experiment with it. This long sack is, perhaps, about 20 feet or 30 feet long and 6 feet in diameter with a big hole in the front distended by a hoop, and we put its head to the wind. The wind blew it out tight, and we all got inside. It was raining at the time, and that is what made us get inside. However, we tested with an anemometer to find which way the wind blew, because when you are inside you cannot feel any wind. We found, in testing it with the anemometer, that the wind blew straight in at the front aperture, but anywhere else it blew in the opposite direction. The result was that just close to the surface of the apparatus the wind was blowing along its surface, first of all practically at right angles to the wind, and then in the same direction as the wind. I think it is a most interesting subject, and one that requires a good deal of investigation. I am not convinced that it can be applied to any very practical purpose, but Mr. Alexander thinks it can, and I hope he will show us how it can shortly.

Observations and Experiments relative to Equilibrium in Air of a Body Heavier than Air.

By Major L. S. Blackden.

On the 2nd of April, 1898, I went out to Sierra Leone on board the *Cabenda*, and after spending six months there I crossed to Jamaica,

where I remained till 1902. During the whole period I took every opportunity of observing the phenomena of soaring.

On the voyage from Liverpool we were followed for some distance by gulls. The wind during that time was on the starboard bow. The gulls flew all round the ship, but remained chiefly in a position a little astern of her. They constantly gave examples of steady soaring, but the soaring was always done in one place, namely near the port quarter, at a point where there must have been the crest of a rising wave of wind flowing over the ship. They showed no signs of any ability to do more than glide at any other place.

In Sierra Leone I was quartered at Mount Auriol, a hutment on a long ridge of hill sloping down from the end of the high ground that culminates in Leicester Peak. The wind often blew straight across this ridge, but the vultures* soared only on the windward side of it. I once saw a vulture, which was soaring steadily above the crest of the ridge, get to leeward of the hospital roof, where one might expect broken currents with a downward trend. His difficulties were obvious, and he had a violent struggle to retain his balance, and to regain a position in the steady updraught that followed the slope of the hillside.

I did not look out specially when in Africa for examples of soaring in a horizontal wind, but Sierra Leone is mountainous, and, therefore, from whatever quarter the wind may come, there is little likelihood of its being horizontal. My hut was on the seaward side of the hill, and my observations from it took place when there was a sea breeze blowing, and, consequently, there was always a presumption in favour of an updraught whenever I saw a vulture soaring.

In Jamaica I was quartered just north of Kingston in the plain of Liguanea, which rises gently from the sea to the mountains, at the rate, roughly, of a hundred feet a mile, and going due north from Kingston, the foot of the hills is reached in about six miles. Every day in summer a strong sea breeze blows which must have an upward trend over the whole plain and the southern sides of the hills. At night a land breeze blows, which certainly has a downward trend, coming down, cool and refreshing, from the mountains, and flowing southwards to the sea. Sometimes, in winter, a north wind blows over the whole island, which, like the night breeze, must have a downward trend over the plain of Liguanea.

I have seen vultures* and large hawks soaring on both sides of the hills, north and south, but always on the windward side, never once on the leeward side. Until early in 1902 I never saw a vulture soaring over Kingston or the Liguanea plain during a north wind or a land breeze, neither have I ever seen a vulture soar in what was undoubtedly still air. I have also never seen a frigate bird or pelican soaring continuously when well away from the land, without a few occasional beats of the wings, though I have read that the albatross soars continuously over the sea, where apparently there could have been no upward trend in the wind, but I should like to see it myself. Every ship and every wave may form a wind break, with an updraught on its windward side, and there might also be rising columns of air over considerable areas of sea.

I have, however, seen a vulture soar when the air appeared to the senses to be absolutely still, but in every instance the sight of a soaring vulture over the plains was followed in a few minutes by the rising of a breeze from the sea. One hot and sultry morning when the air was so still as to be almost stifling near the ground, I noticed a vulture circling round and round on outspread wings just clear of the tree-tops behind my house. He was evidently trying hard to rise, but each completed circle left him no higher than he was at its commencement. I watched him for some minutes, till at last one of his circlings brought him out with a distinct gain in elevation, and almost at the same moment I felt upon my cheek the first breath of the coming breeze. The sea breeze soon was blowing over the plain, and the vulture soared easily aloft.

Early in 1902 I saw what I had never seen before, namely the soaring of a vulture over the Liguanea plain, which slopes from the hills southwards to the sea, when a north wind was blowing. As far as I could see there was not a bird in sight, except near a particular barrack building, where, to my own great surprise, I saw two vultures circling on the windward side of the roof. Where they were the wind had evidently an upward trend, but I had never before seen any soaring done unless the general trend of the wind over the whole plain was a rising one. The two birds were evidently trying hard to rise and get away. I watched them for a long time, during which they failed entirely to get any higher than where one might presume the crest of the wave of wind flowing over the building to be.

* Neophron Monarchus.

* Cathartes Aura.

At last one of them gave it up as a bad job, and gliding off to a large tree, settled among the branches; but the other kept it up, and finally, by slow degrees, so far improved his position as to be able to enlarge his circlings, to show less appearance of effort, and then to soar away till he was only visible as a speck in the distance, and at a great height in the sky. There was no immediate change of wind, so I have nothing to go upon to account for his success; but in view of all my other observations, I could only suppose that there was a stratum of wind from the sea at no great height above the roof of the building. Neither bird attempted to rise by beating his wings.

As a result of these observations I came to the conclusion that soaring was impossible unless there was a rising trend in the wind, and that the angle at which the wind must rise in order to make soaring possible depended on the strength of the wind and the head-resistance offered by the bird. I thought that possibly the separated pinions of the vulture might offer greater resistance to the air than the pointed wing-tip of the gull, as each primary feather of the vulture was bent to a different extent, so that each separate feather had to force its own way through the air, and that, therefore, the vulture was better fitted for soaring over the irregular surface of the land than over the more level sea, but I have seen nothing to support that idea.

On the voyage to Sierra Leone I found that the gulls invariably soared with the tips of the wings depressed to a point at least as low as, and generally lower than, the roots. The rough sketches I made at the time show wings rising from their roots, but with the ends for more than half their length depressed till the tips were as low as the centre of the body, or even lower. The extreme ends of the wings were bent upwards so far as to make them horizontal. Vide Figs. 1 and 2.

In Sierra Leone the vultures showed exactly the same thing, but having longer and more separated primaries, the upward bend of the wing-tip was more pronounced. Only when making a sharp turn were the wings thrown back a little so as to make a slight dihedral angle. Vide Figs. 3 and 4, which show the Sierra Leone vulture.

In Jamaica, on the other hand, the vultures held their wings straighter from root to tip, and the two wings thrown back at a distinct dihedral angle, the tips, as in the vulture of Sierra Leone, being bent sharply upwards. Vide Fig. 5. Hawks in Jamaica appear to soar with their wings held in an absolutely straight line from tip to tip. Vide Fig. 6. Swallows have their wings depressed, and all non-soaring birds glide with depressed wings.

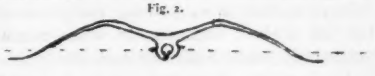
I was in Jamaica three years and a half, against six months only in Sierra Leone, so it is possible that I may have missed seeing in Sierra Leone what was really there, and only noticed it when it happened in Jamaica, but I had ample opportunities in Sierra Leone, and my impression is that the soaring of the Sierra Leone vulture, with his depressed wings, was much steadier, and the equilibrium apparently more easily maintained in a gusty wind than was the case with the Jamaica vulture, whose wings were thrown back at a dihedral angle. I refer chiefly to transverse equilibrium. Motions of transverse balancing were constantly to be seen in Jamaica. I have seen a gust from the side bring one wing into a nearly vertical position, when its owner rapidly half folded it to get it down again. It seemed to me that raised wings made balancing more difficult in a gusty wind, and I personally always accounted for the dihedral angle between the wings of the Jamaica vulture by presuming that his skill was so great that he could afford to let his muscles lie rather slack, his wings being thrown back owing to natural laziness, a fault not unknown in the West Indies. The vulture that gave up its attempt to rise in the north wind was too lazy to give its wings a single beat in order to accomplish its object.

The gulls on the voyage did not appear to make use of their heads or feet in balancing; whenever either head or foot was moved it seemed to be to serve some immediate purpose other than that of maintaining equilibrium. The head turned this way and that to look out for scraps of food, etc. A splash would be followed at once by every gull turning his head in that direction. I never saw the feet move from under the tail except immediately before settling down on to the water or on to a perch, or else for some obvious purpose, such as scratching the head, which I have seen a gull do while soaring. I could not detect any motion obviously meant for fore and aft balancing. The tail was only spread and distinctly made use of when making a sharp turn, or in stopping headway prior to settling. I must add, however, that owing to the gulls always soaring a little astern of the ship, I did not get a very good view of their tails. Among the gulls I occasionally saw one wing give a single beat, the other remaining steady. This I saw also among the vultures of Jamaica.

Both in Sierra Leone and Jamaica I saw distinct movements up and down of the tail of a vulture, and also occasionally a slight re-

Fig. 1.

Fig. 2.



Soaring attitude of Gulls.

Fig. 3.

Fig. 4.

Soaring attitude of Sierra Leone Vultures.
(Neophron Monachus.)

Fig. 5.

Vulture.
(Cathartes Aura.)

Fig. 6.

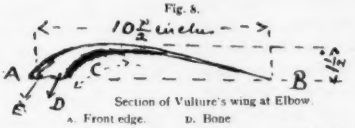
Hawk.

Soaring attitudes of Vulture and Hawk in Jamaica.

Fig. 7

Under-surface of Vulture's wing.

Fig. 8.



Section of Vulture's wing at Elbow.

A. Front edge. D. Bone
B. Rear edge. E. Sinew to support sharp edge
C. Short dark feathers.

Fig. 9.

Section of wing at wrist.

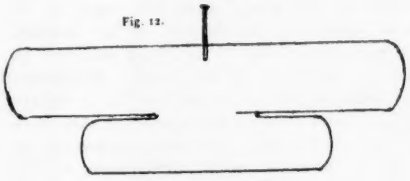
Fig. 10.

Light breeze.

Fig. 11.

Strong breeze

Fig. 12.



Flat glides, with wide tail.

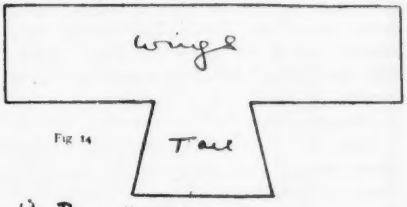
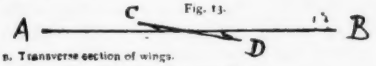


Fig. 14

Shape finally adopted for tail

Fig. 13.



A., B. Transverse section of wings.
C., D. Transverse section of tail when twisted.

Fig. 15.

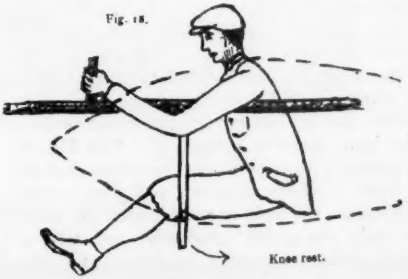
Fig. 16

Main spars upon which the arms rest

Fig. 17.

Knee supports hung so as to swing in all directions, but not to twist round.

Fig. 18.



— — — — Outline of fish-shaped frame enclosing Aeronaut.

volving motion so as to bring the plane of the tail on a slant transversely while remaining apparently horizontal fore and aft.

In Jamaica, in a high wind, I frequently saw a vulture, when (owing to a sharp turn, or a gust from the side), one wing became very greatly elevated, slightly shorten the raised wing, the result being a rapid return to the horizontal.

Just before alighting, vultures and gulls would bring the wing-ends a few inches forward, and this had the effect of tilting them up in front, and so stopping their way. This movement suggests that horizontal fore and aft balancing may be always effected by slight movements forward or back horizontally of the wings. Such a movement would certainly have the desired effect, and need only be slight. I looked out for, but could not detect, such movements when the birds were under way.

It was always difficult, if not impossible, to estimate the angle of the wing to the relative wind. I contented myself, therefore, with observing the relative angle of different parts of the wing.

The angle seemed to be greatest across the middle of the wing in all the soaring birds I saw, that is, across what I might call the wrist, where the carpal bones come at the further end of the radius and ulna. Near the body the wing-chord seemed more or less horizontal, and at the tip the primaries seemed to be brought almost edgewise through the air.

There was one noticeable thing about the under surface of the wing of the Jamaica vulture, and that was that the short soft feathers underneath the leading edge of the wing were of a much darker colour than the bulk of the under surface of the wing. Vide Fig. 7.

Now, I had read, in the *Aëronautical Journal* for July, 1899, of some experiments by Mr. A. A. Merrill, of Boston, U.S.A., which tended to show that the short feathers on the under surface of the wing in the forward half of it seemed to assist the forward movement of the wing through the air. In the Jamaica vulture they cover the whole under surface close to the body, and only a very narrow strip at the wrist. I am inclined to suggest that the dark-coloured portion, where the small feathers are, is that part of the under surface which has a negative inclination. Vide Fig. 8. If I am right, then obviously the chord close to the body is wholly negative, and that as one recedes from the body and approaches the wrist one finds the chord, gradually, and with a helical twist, becoming positive, and the hollowness of the under side apparently becoming less. The angle decreases again with the separated primaries, which, as I said before, appear to be drawn almost edgewise through the air. That they are not quite edgewise is only shown by the fact that they are always bent up.

Both in Sierra Leone and Jamaica I had a vulture caught for the sake of measurement and weighing.

The Sierra Leone vulture weighed 3 lbs. 8 ozs. His wings measured 5 ft. 1 in. from tip to tip, but only 2 ft. 6 ins. from wrist to wrist. The width of wing varied from 10½ ins. to 11 ins., being widest at the wrist. The thickness of the front edge at the elbow was almost the same as the height of arc, which was 1½ins. The width of the spread tail at its widest part was equal to its length, namely, 11 ins. The supporting area of the wings was, roughly, 4 sq. ft., showing a weight of less than a pound to the square foot. The Jamaica vulture (two specimens) weighed less, being 3 lbs. 2 ozs. and 3 lbs. respectively. The stretch of wing was almost the same as in the African bird, namely, 4 ft. 10 ins. and 5 ft. respectively. The width, thickness, and height of arc of the wings were the same in all three specimens, but the tails of the Jamaica specimens were only 9½ ins. long and wide in one case, and 9 ins. in the other. Altogether the Jamaica vulture seemed to be a lighter bird in proportion to the supporting area than his African brother. The cross section of the wing at the elbow in all three cases was as shown in Fig. 8. This shows a height of arc equal to one-seventh of the chord. The highest part was a little behind the greatest thickness, and altogether the section agrees wonderfully with Mr. Merrill's most successful experimental section. Across the wrist the section was simpler, rather fish-like, and with a much flatter curve. Vide Fig. 9.

The outline of the wings of a soaring vulture as seen from directly beneath him, differs according to the strength of the wind, but in every case the wrist-joint is considerably in advance of the elbow-joint, and of the point of junction of the front edge with the body. Figs. 10 and 11 illustrate the effect of strength of wind on the posture of the wings. In both cases the flatter portion of the wing at the wrist, which has the greatest angle above the horizon, is well in advance of the more deeply-curved portion, whose chord has a smaller angle with the horizontal, and, in parts, a negative angle. When the wind is strong, the wing is shortened, and the wrist placed more forward in comparison to the deeply-curved

Photo. by Elliot & Fry.]

THE LATE MR. JAMES GLAISHER, F.R.S.

part between the shoulder and the elbow than is the case in a light breeze. The flattest angle at which a vulture could glide in still air was very difficult to determine. Before alighting, even when there was no perceptible breeze near the ground, a vulture would sail along close to the ground, on an absolutely level course for twenty or thirty yards. This was accomplished, I presume, by increasing the angle of his wings as his speed diminished, by gradually bringing his wings-ends more forward. After much watching, I came to the conclusion that a descent of about one in fifteen might represent a fairly approximate estimate of the best a vulture could do, but I am not at all sure about this point.

Both in Sierra Leone and Jamaica I tried experiments with small gliding models. There was no skilled labour available, I had no adequate accommodation, my own manual skill was not great, and I was shy of publicity in making my experiments. Each model had to be thrown from the hand, and had to have many falls before its centre of gravity was properly adjusted, and the resistance and lift of the wings equally balanced on either side of the centre of gravity. Under these circumstances I got the most satisfactory results from little gliders of paper or thin card, weighted first with a pin, and later, with a wire nail.

There was little of a quantitative nature about either my observations or my experiments. My object, in both, was to acquire some idea of the proper principles to go on in order to best attain a safe equilibrium in a larger machine.

My first little gliders were made of note-paper weighted with pins; I found that in such small models, generally less than six inches across the wings, the resistance to advance was very great in proportion to the lift, and, consequently, in proportion to the weight carried, and so, perhaps, I attached undue importance to the results of simple head resistance. In fact, this was so much the case, that my first experiments led me to think that equilibrium depended entirely on head resistance, but I soon began to recognise the results of many different components of the pressure received from the air.

My wings were plane surfaces, having a dihedral angle, and the tail was horizontal, and had the same dihedral angle. In fact, the whole glider was cut out of one piece of paper and folded down the middle. Pins were stuck through the fold, near the front edge, so that the heads rested on the paper, and the body of the pins being below it. They sometimes flew excellently, but as a rule flew in a curve, circling to one side or the other, in fact, revolving on a vertical axis. The causes soon revealed themselves to be either want of symmetry in the wings, that is, want of equality of lift and drift, especially drift, on either side of the central fold where the pins were hung, or a want of transverse parallelism between the wings and tail, or else simply from the pins not being exactly in the central fold, which had the effect of making the wings unsymmetrical.

I once stuck a small weight of wax on to one of the wings, expecting to weigh it down and make the glider circle towards the weighted side; but, to my surprise, every time I tried it, the glider circled towards the unweighted wing, with the weighted wing considerably raised. This showed that in such small gliders drift was of greater importance to equilibrium than lift.

In gradually increasing the proportion of weight to surface the tendency to turn on a vertical axis increased, and a tendency to revolve on a fore and aft horizontal axis also increased. Owing to the increased speed necessary to carry the extra weight, the effects of air-pressure were intensified. As the revolution on a vertical axis commenced, its inertia tried to carry the glider a little farther on its original course, the result being that the relative wind came a little from the side and initiated a revolution on a horizontal longitudinal axis. It came, in fact, to this, that the little glider was blown over by the wind of its own advance.

A dihedral angle between the wings was evidently a disadvantage when the relative wind came a little from the side, as it always did when the glider began to turn out of a straight course.

I then tried straight wings and slightly depressed wings, and found the equilibrium, as a rule, well maintained with either. With straight wings usually better.

I then tried a vertical keel below straight wings with the weight attached to the bottom of the keel (I thought at that time that equilibrium would be helped by placing the centre of gravity well below the centre of supporting pressure, on the principle of giving a boat a heavy keel), but found the results worse than without a keel. One of the keel gliders was then tried with the weight attached to the upper surface of the wings, over the keel. This gave better results, but when I opened a window and made the glider move across the draught, instead of heeling over to leeward, and circling sharply away from the window, it simply plunged straight down to the floor.

I soon found that this was caused by the pressure of the air on the side of the keel below the centre of gravity heeling the wings over to windward so that the air impinged on their upper surface, and threw the glider down.

It was now getting pretty obvious that the balance of a glider in the air had nothing to do with that of a floating body, and no likeness either to that of a boat in the water or a balloon in the air.

From whatever direction the relative wind happened for the moment to come, if it exercised a force that was unequal, or dissimilar in direction or effect, on any opposite sides of the centre of gravity, it set up revolution of the whole machine about the centre of gravity, which continued until such force had become equally balanced on opposite sides of the centre of gravity, and the result was the same whether the force considered was direct pressure, or any component of pressure.

A tendency to revolve on a transverse horizontal axis, from a small difference of pressure from the front in the direct line of motion above or below the centre of gravity, could evidently be obviated by causing an equal tendency to revolve on the same axis, but in an opposite direction, by moving the centre of gravity further forward, or further back, or by altering the setting of the tail, so as to increase or decrease the pressure on its upper surface, so that if the rule given above showed the true principle of aërial equilibrium, it followed that the centre of gravity might, within smaller limits, be safely raised above the plane of the wings instead of hung below it, and that with a proper placing of the centre of gravity in a fore and aft direction and a proper adjustment of the tail, the equilibrium would be maintained in one case as well as in the other. In fact, for horizontal flight, there was no necessity whatever to place the centre of gravity low down with reference to the supporting surfaces.

I now made my little gliders absolutely flat, and with a wide and short tail, similar in shape to the wings, but on a smaller scale. *Vide* Fig. 12.

I had found, previous to this, that when a glider persistently circled to one side, a slight crumple of the edge of the advancing wing would make the glider go straight by adding to the resistance of that wing. In the new shape the tail was made similar in outline to the wings but on a smaller scale, and the fold or crumple, when required, was applied to one or other side of the tail. I found that when both sides of the tail were so folded as to bring the pressure entirely on one surface of the tail, the glider would only fly with the pressure on the upper surface of the tail.

Having, therefore, reached the conclusion on the subject of equilibrium given above, I proceeded to stick a pin into the upper side of the flat glider, with only the point in the paper, and the head nearly the whole length of the pin above the plane of the wings. The glider so treated, with the tail bent slightly up to the same side as the pin, flew perfectly, gliding as steadily with the pin standing above it as ever it had done with the pin hanging below it.

I tried the same thing with a wire nail, and after the usual trouble in adjustment for symmetry and fore and aft balance, the nail was carried upright across the room as well as the pin had been. The only drawback to the raised centre of gravity during horizontal flight appeared when I caused the glider to cross a draught. The glider with the raised centre of gravity was liable to be thrown straight down, while that with the lowered centre of gravity heeled over to leeward, and circling rapidly away from the window generally came down in a spiral. This was a confirmation of the theory that force should be equal and symmetrical on any opposite sides of the centre of gravity, for, in the first case, all the lateral pressure was felt below the centre of gravity, with the result that the wings were, so to speak, blown away from under it, so that the glider heeled with the windward wing down and received pressure on the upper surface of the wings.

In still air, having once fixed the weight, either above or below the wing plane, I could make the glider go either way up, by simply altering the setting of the tail. It would always fly, no matter which side the weight was, with the tail turned up.

As I have already stated, in order to pervent a glider circling to one side, that is, in order to obtain practical symmetry of the two wings, I used to make a slight fold in the edge of the advancing wing, or of the tail on the same side as the advancing wing, to add to its resistance.

I now found that, while keeping the resistance on both sides as low as possible I could always make the glider go straight by slightly twisting the upturned tail so that one side, either the right or the left, was slightly higher than the other. Fig. 13 will show what I mean. This did not add at all to the resistance of the tail, but it added a small lateral component to the pressure received on the upper surface, and which necessarily acted in a direction normal to that surface. After this I could decrease the resistance by doing away with the wide tail, made in imitation of the wings, and return to a shape more like a bird's

or fish's tail. *Vide* Fig. 14. The setting of the fore and aft position of the centre of gravity was rendered much easier by the use of the upturned tail, as a small error could be at once rectified by altering the angle at which the tail was turned up. In fact, in still air the tail could be made to largely govern the equilibrium, but when a draught struck the glider the equilibrium depended entirely on the position of the centre of gravity with reference to the force exerted on the whole glider by the wind.

I have already mentioned the increased difficulty of getting a good glide when I increased the proportionate weight carried. The only remedy to this was found in bending the wings, so as to give them a curved fore and aft section with the hollow side on the under surface of the wings.

As the weight carried per unit surface was increased, so the curve of the wings had to be increased, showing that the deeper the hollow on the under surface, the greater was the carrying power. The extreme load to which I finally attained, and to which I shall refer later, was on a little glider of smaller dimensions than any of those which had hitherto carried a single pin, but which, with a hollow wing, whose rise of arc was about a quarter of the length of the chord, carried a nail weighing 28 grains. I found that the efficiency of the tail appeared to be increased by curving that also, with the hollow side up. At any rate I constantly made gliders, with the length of the tail, from the posterior edge of the wings to the end of the tail, no greater than the width of the wings from front to rear, which was usually from a fourth to a sixth of the tip to tip spread.

I found just as good carrying capacity from a flat wing with the front edge bent sharply down, as from a wing having a circular curve in the fore and aft section, but later experiments with rather larger gliders do not bear out this conclusion.

With reference to the shape of the wing section some of the gliders seemed to show that the right place for the centre of gravity in a fore or aft direction was about in line with the highest point of the arc. With a circular curve, one of the gliders flew best with the fore and aft balance about in line with the centre of the wings. Another glider, with the wings so curved as to bring the highest point of the arc to a place a third of the way back from the front edge, required the point of balance to be brought forward till again in line with the highest part of the arc. I only tried that question on two or three gliders. My smallest and heaviest glider seemed to belie that conclusion, which was suggested by the performances of two larger and very light gliders. I only mention it because it seems reasonable to suppose that a current of air flowing over the under surface should exert its greatest lifting effect where its course is forcibly deflected downwards.

I now made all my gliders perfectly straight transversely, from tip to tip of wings, with a curved fore and aft section of both wings and tail as described already, and with the weight, which was a wire nail with the head cut off, lying horizontally fore and aft between the wings so as to bring the centre of gravity as near as possible, vertically, to the centre of supporting pressure.

Having made the glider as symmetrical as possible, and then having, if found necessary, slightly twisted the tail, as already described, so as to make the glider fly straight in still air, I next proceeded to open the window, and stand opposite to it, and some distance from it, usually the opposite side of the room, so that the glider, being thrown towards the window, could not pass through it unless I had thrown it evenly, and it was gliding straight and steadily. On these occasions there was generally a good breeze blowing. In Sierra Leone the breeze came straight up the hillside and into my window. In Jamaica it usually blew across the window from right to left. I cannot say what the speed of the wind on an average was, but in Jamaica, when the sea breeze blew, there were crests of foam upon the waves and dust clouds blown up from the roads, and it was that breeze, broken by trees and houses, which crossed my window as I flew my glider.

In every instance, when the glider, made on the principles described, flew straight and clear through the open window, it flew almost like a thing of life outside. Now rising, now falling, circling a little to one side and a little to the other, but ever making headway against the breeze, except in the strongest gusts, and gradually losing elevation till it settled to the ground. Once, so strong a gust caught the glider square on the right side the moment it cleared the window, that it was carried bodily some thirty feet to the left in a fraction of a second. I, of course, lost sight of it at once, but I rushed to the window, and there it was, a long way to the left, but gliding steadily on an even keel, heading into the wind and *rising*. Its course soon brought it close under the branches of a tree, where a downward eddy brought it at once to the ground.

The fact seemed to be that a glider made on the principles of offering equal and similar reaction to air pressures on opposite sides of the centre of gravity flew as well in a strong and gusty breeze as in still air, and that automatic equilibrium was practically achieved, with no moving or movable part.

Carrying the loading still further, and at the same time increasing the fore and aft curvature of the wing, I reached at last a little glider which had about 1.4 square inches of supporting surface, and carried a nail weighing 28 grains, which made the load nearly half a pound per square foot. The weight of the cardboard wings and tail, and of the little piece of thin paper and glue used to fix the nail, was not ascertained, but the nail alone weighed, in proportion to the surface, at the rate of 6½ ozs. to the square foot. The total weight, therefore, was probably from 7 to 8 ozs. to the square foot. This load on so small a glider was enormous. I have got no record of the height of arc of the wing curve, but it was, as far as I can remember, something like a quarter of the width of the wing.

I spent half a day indoors trying to make it fly, but adjust it as I would it turned a somersault within six feet of my hand every time I threw it, and so rapidly, that I could hardly see on what axis it turned, but I think it was always on a horizontal fore and aft axis.

Next day I tried dropping it from a height of twenty feet, so as to obviate any risk of an unskilful throw. Luckily it was a very still day, so I could drop it from an upstairs verandah out of doors, but still it was no good, and I spent the day in failure. It struck me that some new cause of upset might be present, and that probably that cause lay in the deep curvature of the wing. Remembering that birds whose wings are deeply curved near the body, like vultures, always soar with the flatter part, at the wrist, well in advance of the remainder, I thought that perhaps a small aëroplane attached to each wing-end, in front of the wing, and having a greater angle above the horizon than the rest of the wing, might prevent the deep curvature from upsetting the glider. I tried it, and though it seemed to assist a little, I recognised at once that it was practically impossible to ensure that the two little planes should retain under pressure that exact symmetry, and equality of upward angle, which was necessary to ensure equilibrium.

I accordingly removed them and placed a single plane in front of the centre. It was inclined so as to get pressure on the under surface, the angle being greater than that of the wing chord anywhere. The first throw was successful, and the little heavily-loaded glider flew steadily to the opposite end of the room. The small front plane helped, of course, to support the weight, but as it measured a little less than a quarter of an inch square, it could not make much appreciable difference in the proportion of weight to surface. By twisting this forward plane or rudder so that the transverse section was not quite parallel to that of the wings, as I had previously done with the tail, I found I could cause the glider to go straight or circle to the right or left as I liked.

There is one effect of air pressure that I have omitted to mention, namely, that when I used wings curved fore and aft, but straight transversely (as I always did latterly), if for any reason the glider flew at a worse angle than usual (such as from the tail being turned up too sharply, and receiving in consequence too much pressure) so as to bring the relative wind too much under the wings, it would rock constantly from side to side. When such rocking was slight it seemed to do no harm. I have had a glider pass out of my window into a fairly strong breeze, and keep its balance perfectly, rocking all the time, although in extreme cases, and with very deeply-curved wings, the rocking may be so bad as to upset the glider altogether in still air.

In theory I liken a gliding machine to an arrow. The heavy head, which has great inertia and little surface, is least affected by the wind. The feathered shaft, which has less inertia and greater surface, is always kept, by the wind of its own movement, behind the head. It would be impossible to make an arrow fly sideways through the air. A gliding machine with the centre of gravity much above or below the centre of resistance, is like an arrow going sideways, its point upwards in the first case, downwards in the second.

If the centre of gravity is to one side of the centre of resistance, it is like an arrow going sideways through the air with its shaft horizontal. If the centre of gravity is to one side of, or in front or behind, the centre of lifting effect, the machine is like an arrow falling sideways through the air, and in a large machine the lift is far the most important of the forces to be balanced. The simile may be considered a little exaggerated, but it helps one to understand the necessity for balancing all forces and impulses received from the air on any opposite sides of the centre of gravity if revolution is to be prevented.

There is, of course, one permissible revolution, and that is, that as the centre of gravity is necessarily in front of the centre of surface of the wings, to say nothing of the tail, a per-

fectly balanced machine will always tend to head into the wind—like an arrow.

The importance of having the centre of gravity in the plane of the wings is especially evident when a gust strikes from one side. If the centre of gravity is above or below the centre of effort of a gust from the side, it must tend to upset the machine. If, on the contrary, it is placed in line with the centre of effort, the gust will simply carry the whole machine, on an even keel, a little to one side, heading it at the same time towards the direction from which the gust came.

Hence it follows that a dihedral angle between the wings is bad under the circumstance of a gust from the side, because, in the first place, the gust will strike the under surface of the windward wing, and the upper surface of the leeward wing, and though the forces may be equal, they are certainly not similar (as one is up, and one is down), on opposite sides of the centre of gravity, and in the second place as the centre of gravity cannot well be much above the point of junction of the wings, and is, therefore, below the bulk of the wing structures, the effort of the gust will not be equally balanced above and below the centre of gravity. The centre of gravity will be obliged to move with considerable rapidity in order to prevent a complete capsize.

A dihedral angle is, of course, very good when the relative wind comes from below the wings, as when falling, or gliding downwards at a steep angle, or stopping headway by tilting the machine back, but when in forward flight, if we neglect for the moment the effect of gusts, the relative wind comes from the front, and if the machine turn till one wing is nearly vertical, and the other nearly horizontal, as long as the line of flight is horizontally forward the pressure on each wing will be equal, and normal to its surface, and there will be nothing to prevent a complete capsize.

Of course, if such a position of the wings was really acquired during flight, the force of the air pressure, acting normally to the surfaces on which it impinged, would now have three components, namely, lift, drift, and a lateral component, and as these components act through the centre of pressure, they all act in advance of the centre of surface. Lift and drift are balanced by weight, but the lateral component is not affected by the fact of the centre of gravity being at the centre of pressure, so that the whole machine, as long as it remained tilted as I suppose, would infallibly circle round towards the depressed wing, and inertia would cause a greater pressure on the raised wing than on the depressed one. In addition to that the wheeling wing would be travelling faster than the pivot wing, which would bring yet more capsizing pressure on it. The loss of direct support from one wing would tend to cause the machine to fall, and so bring about the one useful effect of the dihedral angle, but I fear the machine would have capsized too soon for that.

An aërocurve straight transversely is not so good when falling, but it is better when travelling. Even when falling, if one end falls the faster, the centre of pressure will move towards that end, and right the machine, because, in falling, the lowest wing-end becomes the leading edge of an aëroplane moving at an acute angle with the relative wind. The greatest theoretical drawback to an aërocurve being straight transversely ought to be seen when a gust strikes it from one side and with a downward trend. It will strike on the upper surface, and as the windward wing-tip has become temporarily the leading edge, the pressure of the wind will depress the windward wing, and cause a very rapid fall of the machine. Practically, I have never found this happen.

If the outer ends of the wings have a considerable angle above the horizon, a gust that is not quite square from the side—and such a gust cannot be of common occurrence—will still have a chance of striking the under surface, but I would suggest, as the best transverse (tip to tip) section, one of those shown in Figs. 15 and 16.

In either of these cases a gust from the side might have a lifting effect, even with a downward trend of 8°, supposing the curve to have a rise of arc equal to one-twelfth the chord. *Vide* table on page 104, Vol. 25, *Encyclopædia Britannica.*

In Fig. 15 both ends of each wing are to be on the same level, and the centre of gravity, if anything a little above the junction of the wings, but below the top of the arc.

As the centre of pressure has a smaller travel under curved surfaces, there is an additional reason for having the wings arched in both directions.

The longer the tail is, that is, the farther the centre of pressure on the tail is behind the centre of gravity, the more power it has over the equilibrium, but, owing to the increased leverage of its weight on the body of the machine, its construction must be lighter than is the case with a short tail, so as not to interfere with the fore and aft position of the centre of gravity. Therefore the longer it is the weaker it must be. The late Mr. Percy S.

Pilcher, and I believe Herr Lilienthal also, lost their lives through the breakage of the tails of their machines.

Being then dangerous to give the tail a long leverage, we should make up for it by giving it a better grip on the air, and this, I think, is accomplished by giving it a curved section fore and aft with the hollow side up.

For the same reason, namely, adequate strength combined with efficiency, the tail should be fixed rigidly to the machine.

If the centre of gravity is so placed that gusts have the minimum effect in initiating a revolution about the centre of gravity there is no longer such urgent necessity to at once head the machine into the wind, and the necessity for a vertical rudder vanishes, or, at any rate, diminishes.

It seems to me that automatic equilibrium, obtained by means of automatically-moving parts, must be, generally speaking, on one of two principles, namely, mechanism calculated to maintain the machine always on an even keel, such as pendulum or gyroscope mechanism, or such an arrangement of movable parts, that the gusts themselves have the effect of heading the machine into the wind before it has time to capsize.

The first arrangement, among other drawbacks, would have the effect of offering resistance to any attempts to change the direction of the machine by an alteration of balance, as has always hitherto been done in gliding machines, and as seems to be the soundest way. In the second case, as the commencement of any change of direction, combined with the effect of inertia, would result practically in the wind of advance coming from a new direction with reference to the machine, the automatic mechanism would be all the time trying to bring the machine back to its original direction. In other words, the more perfect the automatic equilibrium is, the less easy will be the manœuvring and management of the machine, and in any such machine it would be necessary for the aëronaut to be able at any moment to assume control of the automatic mechanism. Another drawback to moving parts is their comparative weakness, which is very serious when it is so necessary to cut down weight to the limit of safety. Up to now, so far as I know, no one has been killed in recent times by an upset brought about by the wind, a fatal fall has hitherto resulted only from a breakage.

Still, another drawback to moving parts for automatic equilibrium would be the great difficulty of so accurately adjusting their mobility as to make them react, with exactly the right amount of force to each and any of the disturbing impulses. Mr. Chanute has said (*Aëronautical Journal*, October, 1901) that "the proper adjustments are most delicate, and would require years of experiments, and thousands of 'cut and try' experiments."

In place, therefore, of aiming at automatic equilibrium, I would suggest that the best solution of the difficulty is to have a machine so made that, first, a gust from any direction should have the least disturbing effect on its equilibrium, and, secondly, that a small movement of the aëronaut should have the most powerful effect upon it.

The first desideratum can be obtained, as I have endeavoured to show, by the proper placing, vertically, of the centre of gravity, and the proper shaping of the parts most affected by the gusts, namely, the wings, and, in a lesser degree, the tail.

The second desideratum requires the employment of supporting surfaces of the smallest possible horizontal dimensions, which can be effected by superposing small surfaces, or by employing very deeply-curved ones. Of these two methods of giving the greatest effect to a small movement of the aëronaut, the superposed surfaces would be best, if their use did not make it almost impossible to properly place the centre of gravity vertically without bringing the lower surface too near the ground.

The deeply-curved surfaces have hitherto been considered too dangerous, but I have suggested a means, namely, the use of a front rudder as already described, which I hope would make their use safe. I do not propose a greater proportion of height of arc to length of chord than one in seven.

An additional method of giving greater value to the movements of the aëronaut would be to cause such movements to actuate a subsidiary rudder, or rudders, of small size, and the breaking of which would not seriously compromise the stability of the machine. For instance, while leaving the greater part of the front rudder rigid, a piece at each side might be made movable so as to add to the effect of the movements of the aëronaut. This last suggestion was made tentatively and hesitatingly. I have since seen that it is not only not new, but has been tried with success by the Messrs. Wright.

In a machine driven by a motor, I think the principle of superposition would be the best. The placing of the motor itself sufficiently high in the machine would help to bring the centre of gravity into the right position without bringing the lower surface too near the ground. In a pure soaring machine a single surface as

deeply-curved as would be safe without unduly increasing the size of the front rudder, would, perhaps, give the best results. The front rudder should be smaller than the tail.

In a motor-driven machine, the line of propulsion should pass through the centre of gravity, and the latter should be placed as in a gliding machine, so that, whether the motor were in use or not, there should be no effect of driving an arrow sideways through the air. In other words, all forces of air pressure should be equal and symmetrical on any opposite sides of the line of propulsion.

The vulture, as I said before, can, I believe, glide in still air for an indefinite distance with a fall of one in fifteen. At any rate, the flatter the angle of descent, the more numerous would be the occasions when an up-draught had sufficient strength and angle of rise to make continuous soaring possible. The only means of improving the angle of fall is to decrease the head resistance, as the angle must always be in the proportion of head resistance to weight when once the machine has attained soaring speed. The only conditions of reducing head resistance are, first, to so shape the supporting surfaces that they can sustain the total weight when opposed to the air at the flattest angle, and at a moderate speed; secondly, to reduce the framing, struts, stays, etc., to the lowest limit compatible with adequate strength; and, third, to reduce the resistance of the aëronaut's body.

The deeply-curved wing, and the proper fore and aft balancing of the machine would help to fulfil the first condition. The use of the wing section suggested by Mr. Merrill, already referred to as being so like the vulture's wing, and which has such a large enclosed space within the wing as to permit of a very strong construction, no part of which would have any effect on head resistance, so that the outside struts and stays could be much reduced, and possibly in time abolished, would go far to fulfil the second condition.

With regard to the third condition, I notice in the picture of Chanute's two-surface gliding machine given in the article on aëronautics in the new volumes of the Encyclopœdia, that the aëronaut has his knees drawn up to a level with his body, which is sound, as far as reduction of head resistance goes, but too tiring for continuous soaring. Perhaps a stick, free to swing in all directions, might be hung from the frame, on each side, and a little in front of the aëronaut, with a crook at the end long enough to support the knee, so as to enable him, when well started, to get into a more restful position, and yet leave him free enough for all balancing movements. Fig. 17 shows the kind of thing I mean. Fig. 18 suggests what I consider the best position for the aëronaut to assume during flight—like a cyclist with his feet on the foot-rests, and his body bent over the handle-bars.

The difficulty with the knee-rests suggested would probably be more felt, in quickly disengaging the legs from the rests than in getting them up. All these suggestions are to be regarded as suggestions for experiment, and no more.

I do not think it would be advisable, in a practical flying machine, to assume a prone position, but everyone who has coasted down a hill on a bicycle will know the effect of raising the feet and bending the back, in other words, of diminishing the vertical height, in diminishing head resistance in going through the air.

Having then, as far as may be found practicable (by means of rests or seat suspended from the machine or otherwise), diminished the vertical height of the aëronaut, I would further suggest adding a light framework, covered with goldbeater's skin, and formed like a fish with a pointed nose, and with a large hole above and a larger one below for the aëronaut's head and legs, to enclose his body. In this way we would more or less fulfil the third condition for so diminishing the angle of fall as to give a good chance of motorless soaring, and also of reducing, as much as possible, the horse-power needed for horizontal flight. With reference to another desideratum for human flight, namely, being able to start up from anywhere, I should be quite contented, for the present, to be obliged to start from the top of a hill, if I could fly when once I had started, and alight in safety when I wanted to; but I should want to start without assistance, and to alight on my legs.

Before concluding, I should like to protest against the way so many people have of taking out a patent directly they acquire an idea. It is within the bounds of possibility that the unhappy maker of the first successful flying machine may find that almost every part infringes somebody's patent, a patent which, by itself, had done no good to aërodromies.

In such an "inchoate art" the surest road to progress is for everyone to put his ideas into the common stock. It will not be the first successful soaring machine that will bring a fortune to its inventor, and I agree entirely with Mr. Hargrave, that every experimenter, everyone with opportunities for observation, should communicate all he has found out, or thinks he has found out, to all others interested in the subject, and so, by all working together in a common cause, we shall the sooner win

that empire over the air which it is the object of our Society to achieve.

That is my excuse for writing this paper.

Mr. Spencer said that it was undoubtedly on the lines of the aëroplane, and also of the kites that the Society have taken up, that they would really look for the eventual solution of the problem of flying. He mentioned that while these experiments with kites and machines heavier than air are progressing, experiments would be made with a navigable balloon having a motor six times the horse-power used by him and his brother last year.

The CHAIRMAN: I think there is only one duty remains to me, and that is to propose a vote of thanks to the readers of the papers to-night. I have already said a few words about Mr. Alexander's paper, and I can only say that the paper we have heard from Major Blackden is one of very great importance, and I have been very much interested in it. The subject of soaring flight is one which is of great importance, because, as most of you know, some experiments have been made in America with soaring machines, and if it can be shown that flight can be accomplished without any mechanical motion of the wind and there is no necessity for propellers to be carried, the experiments will not have been made in vain. From these small propellers Major Blackden has been experimenting with we may be able to learn more than from the man-carrying machines, which are more difficult to build and experiment with. In talking of soaring birds, it occurred to me that it has often been stated that soaring birds move round in circles, and the reader of the paper referred to that. But now—I am speaking entirely from memory of a good many years ago—I have an idea that pelicans do not necessarily soar in circles. I think I remember seeing large flocks in Egypt going in a perfectly straight line without any motion of the wings. I do not know whether Major Blackden may have ever seen any birds soaring straightly, because that is rather a point one would like to thrash out. Certainly a soaring bird as a rule moves in a circle, and apparently has some object in doing so. I have mentioned these experiments in America, which have been conducted by quite a number of people, but more recently by the brothers Wright, and it is a very great pity we have not here Mr. Chanute, who is to arrive in England, I believe, in a few days' time. He might have explained what are the latest experiments of the brothers Wright, because he has been associated with them. I do not know how long he is likely to remain in England, but I hope we shall have an opportunity of hearing him give some small account of these experiments. I now ask you to accord your thanks to the readers of the papers to-night.

The vote of thanks was carried by acclamation, and the proceedings terminated.

NOTES.

The Permanent International Aëronautical Commission. — The Permanent International Aëronautical Commission have passed a resolution approving M. C. Canovetti's researches into the resistance of air, and emphasising their value to both aërial locomotion and commercial transport generally. The Commission extols two special ascents of MM. Balsan and Corot and MM. de la Vaulx and Borot; by means of balloons and ballonets (equilibreurs) they remained up from 27 to 28 hours. The Commission, on this occasion, are full of commendation. They appreciated M. Teisserene de Bort's valuable meteorological work, and dwelt on his recent departure in Denmark, where his kites have already flown to 3,500 feet and remained at that height with their registering instruments for a period of 72 consecutive hours with fruitful scientific results. The President, Professor Hergesell, pointed out to French aëronauts what valuable additions to our knowledge of the migratory habits of birds might be gained by observation balloon ascents at this season of the year.

M. Santos Dumont's New Airship.— Although the experiments of M. Santos Dumont have, like those of his predecessors and contemporaries, emphasised the limitations of the navigable balloon, there is undoubtedly a fascination about the man and his work. When his new airship, which is to be large enough to carry several persons, is completed, the eyes of the world will again turn to the hero of the Eiffel Tower, and all will expect that if anyone can extend the capabilities of navigable balloons it will be the bold and ingenious Brazilian. There is, at any rate, genius and novelty in all his work, and even if the new airship does not achieve much more than its predecessors, we may hope for something of interest in the renewal of the experiments.

The International Balloon Ascents.— This useful work is being continued with unremitting zeal. In the ascents of October 2nd

and November 6th, 1902, no less than eight nations participated—Austria, France, Germany, Italy, Russia, Spain, Switzerland, and the United States. In October, when the ascents were made in an area of low barometric pressure, the highest altitudes attained by the ballons-sondes were: Strassburg, 13,700 metres, minimum temperature —51°.6C., 5°.2 on the ground. Berlin, 13,930 metres, minimum temperature, —44°.2 at 9,214 metres, 4° on the ground. On November 6th, when an area of high barometric pressure lay over N.E. and E. Europe, the highest altitudes attained were Chalais-Meudon, 15,612 metres, minimum temperature recorded, —55°.2, 11° on the ground. Strassburg, 11,300 metres, minimum temperature, —53°.4, 3°.6 on the ground. Berlin, 12,985 metres, —52°.6, 1°.2 on the ground.

Aëronautical Nomenclature.—The *New York Herald* mentions that the Permanent International Aëronautical Commission has given its approval to the following terms descriptive of modern varieties of aëronautical apparatus. A non-steerable balloon is called an aërostat; a steerable balloon, an aëronat; an apparatus that is heavier than air is an aëronef, or appareil d'aviation; an aëronef with one or more propellers on an axle which is vertical, or nearly so, is an heliocoptère; an aëronef formed of one or more sustaining planes, with one or more propellers on a horizontal axle, is an aëroplane; an aëronaut propelled by wings is a "mechanical bird."

The Sahara Balloon Experiment.—The attempts of MM. Debureaux and Castillon to send two unmanned balloons across the Sahara from Gabes, in hopes they would indicate the route to be taken by a large balloon carrying passengers, did not prove successful. The first balloon, which was called the "Eclaireur," fell 21 miles from the point of starting, and was lacerated by the Arabs on the borders of the desert. The second, called the "Leo Dex," went 373 miles, and was 26 hours in the air, but was also captured. According to the *Globe*, M. Leo Dex points out that the balloon must have reached the desert 100 leagues south of Gabes, where it would have caught the regular alize winds, and been carried on to Timbuctoo. He estimates that a similar balloon of 1,000 cubic metres, carrying 600 kilogrammes at a time when it came too near the ground, would easily cross the 2,300 kilometres between Gabes and Timbuctoo.

The End of M. Roze's Balloon.—The ignominious fate of M. Roze's airship is a warning to those enthusiastic inventors who form companies to purchase their patents and finance their experiments. Patience is certainly a virtue requisite in all those who are connected with aëronautics, whether commercially or otherwise. Yet it does not seem to have been a distinguishing feature of the company who purchased M. Roze's patents. It appears, in fact, to have treated M. Roze's balloon in much the same manner in which the Arabs treated the experimental pilot balloon in the Sahara. Paris journals have recently stated that on account of the calls made upon this aërial company, the shareholders destroyed both the balloon and its shed.

The Observation Balloon of the Discovery.—It is interesting to hear that the balloon which Commander R. Scott, a member of the Aëronautical Society of Great Britain, took out with him on board the *Discovery*, has been successfully used. It is hoped that further details of its use may shortly be published in this journal.

REVIEWS.

Die Luftschiffahrt der Gegenwart (Aëronautics of to-day), von Hauptmann Hermann Hoernes. A. Hartleben Wien, reviewed by H. Auxilium Bruce.

The name of this book is a depressing reminder that compound words have a way of making a language look rich. Captain Hoernes draws a distinction between "Aeronautik," which he says are "Ballon-technik," and "Luftschiffahrt," which includes "Aviatik," which is "Flugtechnik"; and we cannot follow him in English, for aërostatics does not cover the field.

I don't think Captain Hoernes has read *Macmillan*, and learnt that any influence the Press possesses "is wholly bad." To him it is still a wholesome pulse of the life politic, and if not the fourth power in Europe, it is the descendant of the days of Görres, and the 'Rheinischer Merkur," *pace* a reptile press in between; and that the people may understand their own interest in aëronautics and enjoy the daily aëronautical allusions in the papers, he tells us, he has written this book. It is a nice book. I am not going to kill "with faint praise"; it is a very nice book. It is the nicest handbook to aëronautics I have read. It is much better than any of the English popular aëronautical books that have come out lately

The illustrations are not so good as those in an English work that appeared last year, they are rougher; but probably that is partly the fault of the paper. And there are a great many more of them, and they are most interesting and much more scientific. The book is scientific as far as it goes, and will keep the man in the street straight. Now our English popular aëronautical books, when they try to be scientific, take the man in the street into a fog. Captain Hoernes is very generous to other nations in his estimate of their work. Perhaps it seems a *petitesse* to even say this when speaking of the work of a scientist, for who are the cosmopolitans if they are not? But, as I said the author thought so much of the Press, I should like to add that in spite of German papers of to-day, he is left a truth-loving, fair-minded Austrian.

Captain Hoernes devotes four pages to wind-resistance, divided into—general, experiments, results, and deductions from the laws of wind-resistance—a very necessary beginning to an aëronautical handbook, and one which English aëronautical writers might well adopt. Four pages do not seem much, perhaps, for a subject which is the crux of the problem, but the author tells us where more can be found, if wanted, in other of his more technical works.

In the chapter on "Steerable Balloons," Captain Hoernes emphasises that on economical grounds such balloons can never be in general use for traffic, and that, to be of use for sporting, military, and scientific purposes, they must attain at least a speed of fourteen metres per second, able to be maintained for three consecutive hours. As usual, he estimates the value of a steerable balloon by its speed of travel; but the measurement of the actual wind-force overcome recommends itself as a more convincing all-round measure of capacity; it is another way of expressing the same thing, but the *varying pressures* to be combated in going against wind give the results more weight. Of course, to get such measurements, the difficulties of location have to be overcome; but these can be overcome, and the results are much more convincing.

There is a well illustrated section on kites and meteorology, that most satisfying modern development of aëronautics. But it seems a pity such a good kite as that of Mr. Cody should have escaped notice, also that of Mr. Dines, which has done much quite late meteorological work, and that of Mr. Anderson, which is now on board the *Scotia*, with Mr. W. Bruce's Antarctic Expedition.

By the way, I am curious to know how the Germans really like their new spelling? What do they feel when they see *Thatsache* written without an "h"? When I read it, I feel as hurt in the eyes as when an American writes me colour without the "u." But I understand that missing out the "u" in colour saves thousands of dollars a year in American printing expenses, so there is nothing more to be said, though I should like to rebel and sit uneasy beneath the Almighty Dollar.

Foreign Aëronautical Periodicals.

(In this list a selection of some of the more notable articles is given.)

L'AÉRONAUTE (Paris).

January, 1903.—Cinquantenaire de la première ascension d'un dirigéable, par L. Giffard. Rapport de M. Cassè.

February.—Analyse et synthèse du planement appliquées au ballon dirigéable, par G. Berthenson.

L'AÉROPHILE (Paris).

January, 1903.—La Déviation Maritime; G. Geo—La Traversée du Sahara par Ballons-Pilotes; Georges Blanchet—Inauguration du Monument à Henri Giffard; Marius Degoul.

ILLUSTRIRTE AËRONAUTISCHE MITTHEILUNGEN (Strassburg).

January, 1903.—Die Katastrophe Bradsky—Die Aichung des Elektronen—Aspirations apparates, von Dr. Herman Ebert.

February.—Ueber die Ursachen des Unfalls des Luftschiffes, von Severo, von G. Espitallier—Die Durchquerung der Sahara Mittelst Ballon-Schlepp-Fahrt.

March.—Leo Stevens und sein Selbstfahrender Ballon—Der Ballon Lebaudy, von G. Espitallier.

WIENER LUFTSCHIFFER-ZEITUNG (Vienna).

January, 1903.—Wilfred de Fonvielle—Hargraves neue Flugmaschine.

February, 1903.—Gummi und Luftballons—Historisches über des Schleppseil—Myttons Flugexperimente.

March.—Der Stand der Flugtechnik, 1902.—James Glaisher.—Von Paris Nech Ungarn-Versuche Mit Tragschrauben; Raimund Nimfurhr—Die Endgültige Lösung des Flugproblems.

Notable Articles.

1903.

Jan. 1. *The Yachting World.*—"Cross Sea Ballooning."
— *Daily Mail.*—"Kite *v.* Balloon."
3. *Standard.*—"Ballooning Experiments."
12. *Daily News.*—"A Lady Balloonist."
13. *Times.*—"Professor Hele Shaw on Aërial Locomotion."
15. *Christian Globe.*—"A Long Distance Balloon Trip."
Feb. — *The Contemporary Review.*—"The Mechanism of the Air," by Rev. J. M. Bacon.
— *Golden Penny.*—"An Aërial Motor Car."
3. *Newcastle Leader.*—"An Airship Expedition."
19. *Daily Telegraph.* — "Kite-Flying Congress."
23. *Globe.*—"Aërial Photography."
27. *The News.*—"Aërial Travelling."
Mh. 27. *Daily Mail.*—"Airship Flights."
— *Echo.*—"A Dinner in Mid Air."
28. *Hampstead Express.*—"Adventures in Cloudland." Lecture by Rev. J. M. Bacon.

Applications for Patents.

(*Made in January, February, and March.*)

The following list of Applications for Patents connected with Aëronautics has been specially Compiled for the AËRONAUTICAL JOURNAL by Messrs. BROMHEAD & Co., Patent Agents, 33 Cannon Street, London, E.C.

878. January 13. PAUL LEBAUDY and PIERRE LEBAUDY. **Improvements in or relating to Airships.**

922. January 14. JAMES ROBERTSON. **A Navigable Airship, with all the novel ideas comprehended in the description thereof.**

1031. January 15. ARTHUR HENRY PHILLIP BLUNT. **Improvements in means for Propelling and Steering Aerial Balloons.**

1104. January 16. HENRY SHEPLEY BOOTH. **Improvements in Aerial Machines.**

1706. January 23. BERTHOLD KUETTNER. **Improvements in Aerial Apparatus.**

2688. February 4. ADRIAN BAUMANN. **Improvements in Apparatus for Supporting Objects in the Atmosphere.**

2826. February 6. GEORGE WELLNER. **Improvements in Aerial Machines.**

2838. February 6. REGINALD WOOD. **Improvements in Heat Engines, Airships, Motor Cars, Aerial Cranes, Railways, Projectiles, Cycles, Ships, and the like.**

3538. February 14. THOMAS MOY. **Improvements in or Relating to Means and Appliances for Effecting Aerial Navigation**

5256. March 6. L. J. ANDERSON. **An Airship.**